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The EPR Experiment: A Prelude to Bohr's Reply to EPR

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1 Einstein, Podolsky, and Rosen's argument

Bohr's (1935) reply to Einstein, Podolsky, and Rosen's (EPR's) (1935) argument for the incompleteness of quantum theory is notoriously difficult to unravel. It is so difficult, in fact, that over 60 years later, there remains important work to be done understanding it. Work by Fine (1986), Beller and Fine (1994), and Beller (1999) goes a long way towards correcting earlier misunderstandings of Bohr's reply. This essay is intended as a contribution to the program of getting to the truth of the matter, both historically and philosophically. In a paper of this length, a full account of Bohr's reply is impossible, and so I shall focus on one issue where it seems further clarification is required, namely, Bohr's attempt to illustrate EPR's argument by means of a thought experiment. In addition, I shall attempt to clarify a few other points which, however minor, have apparently contributed to misunderstandings of Bohr's position. As the title of this paper suggests, an account of these few points does not consitute an account of Bohr's reply, but it is an important step in that direction.

I shall begin by raising several points about EPR's argument, and especially their example of particles correlated in position and momentum. Some of these points have not been sufficiently noticed in the literature.

Let us begin with a standard, but incorrect, story about EPR's argument. Two particles are emitted from a common source, with momenta p and $-p$, respectively. For simplicity, we assume that their masses are the same. Some time later, particle 1 encounters a measuring device, which can measure either its position, or its momentum. If we measure its momentum to be p, then we can immediately infer that the momentum of particle 2 is $-p$. If we measure

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its position to be x, then (letting the source be at the origin) we can immediately infer the position of particle 2 to be $-x$. Now, if we assume that the measurement on particle 1 in no way influences the state of particle 2, then particle 2 must have had those properties all along, because it could not obtain them merely as a result of the measurment on particle 1. But quantum theory cannot represent particle 2 as having a definite position and momentum, and therefore quantum theory is incomplete.

EPR do not make this argument. If they had, Bohr's reply could have been quite short. The short reply is to note that in order to make the requisite predictions, one must know the precise position and momentum of the source. Consider, for example, that you have just measured the momentum of particle 1 to be p. If you do not know the momentum of the source, then in particular you do not know in which frame of reference to apply conservation of momentum. (Above we assumed that the source is at rest relative to us, and so we infered that particle 2 has momentum $-p$.) Similarly, consider that you have just measured the position of particle 2 to be x. If you do not know the location of the source, then you cannot say where particle 2 is. It is 'the same distance from the source' as particle 1, in the other direction, but how far is particle 1 from the source? Unless you know where the source is, you cannot answer this question.

But if you must know the precise position and momentum of the source in order to make the inferences, then the uncertainty principle will always get in the way of EPR's argument. Suppose, for example, that you know the precise momentum of the source. Then you measure the position of particle 1. The EPR criterion for physical reality says:

> If, without in any way disturbing a system, we can predict with certainty. . . the value of a physical quantity, then there exists an element of physical reality corresponding to this physical quantity. (Einstein et al., 1935, p. 777)

But we *cannot* predict particle 2's position with certainty, because we do not (and under the circumstances, cannot) know where the source is.

Good thing, then, that the 'standard story' about EPR's argument is wrong. We can see immediately that something is wrong with it, because nowhere did that story mention quantum theory, and yet EPR are very concerned to present their argument in quantum-theoretic terms (as they should be). Indeed, the first part of their paper rehearses a number of facts about the formalism of quantum theory, presumably so that they can present their argument in a quantum-theoretic context (which is what they do).

EPR continue by considering a generic system of two particles and a pair of generic (but non-commuting) observables on particle 1, A and B. EPR do not then write down a generic version of the so-called 'EPR state'. Instead, they merely point out that as a result

of measuring A on particle 1, particle 2 may be left in one state—call it $\psi_k(x_2)$, as they do—while as a result of measuring B on particle 1, particle 2 may be left in quite another state—call it $\varphi_r(x_2)$, as they do.

At this stage of the argument, EPR might have pointed out that ψ_k and φ_r are eigenfunctions of *some* observables. Hence we would be able to predict, with certainty, the values of two observables as a result of two different measurements (of A or B) on the first system. One would then have to go on to show that those observables need not commute.

Instead of continuing with this generic case, however, EPR turn to a specific example, using the position and momentum observables. Here they do add the idea that ψ_k and φ_r can be eigenfunctions of position and momentum, respectively. To establish this claim, they suppose that the total system prior to any measurements is in the state

$$\Psi_{\mathrm{EPR}}(x_1, x_2) = \int_{-\infty}^{\infty} e^{(2\pi i/h)(x_1 - x_2 + x_0)p} dp, \tag{1}$$

where x_0 is some constant. EPR then show that (1) is a state of perfect (anti-) correlation between the positions and momenta of the two particles: measuring the momentum of particle 1 (hence collapsing the wavefunction for the compound system!) leaves particle 2 in the relevant eigenstate of momentum, and likewise for position.

So why is the 'standard story' inconsistent with this account? We have already noted that the 'standard story' is not quantum-mechanical, but the more important point for us here is that EPR nowhere describe how the compound system is prepared, nor how it evolves in time. Indeed, the notion of time never enters their discussion. The state Ψ_{EPR}—let us call it the 'EPR state'—is a 'snapshot' of the compound system at a time. Moreover, EPR *could not* give us a dynamical description of the situation, because the EPR state cannot be preserved under Hamiltonian evolution (unless we introduce an infinite potential, a point that I will no longer bother to mention).

The reason is familiar, though not usually mentioned in this context. The support of Ψ_{EPR} has measure 0 in configuration space: $\Psi_{\mathrm{EPR}}(x_1, x_2)$ is zero except when $x_2 - x_1 = x_0$, and so it is a line in the (two-dimensional) configuration space for the compound system. Such a state necessarily spreads under the evolution induced by any Hamiltonian. (We are, of course, ignoring the fact that the EPR state is not in $L^2(R^2)$ in the first place. Neither EPR nor Bohr seem to have been concerned about this point.)

Finally, note that there is no Hamiltonian evolution that can take a generic state $\Phi(x_1, x_2)$ to the EPR state (no matter what Φ is). Only a 'collapse' of the wavefunction can produce the EPR state. Hence we must imagine the EPR state to exist at *and only at* the moment of preparation.

EPR's argument, then, is based on such a state. They point out that upon measuring the

position of particle 1, we can predict with certainty the position of particle 2, and likewise for momentum. Of course, only one of the two measurements can be performed, which raises the question whether some modal fallacy has been committed. After all, their argument apparently takes the form:

1. Actually: position is measured for particle 1, and therefore (actually) particle 2 has a definite position.

2. Possibly: momentum is measured for particle 2, and therefore (possibly) particle 2 has a definite momentum

3. Therefore: particle 2 (possibly? actually?) has a definite position and a definite momentm.

In this form, the argument is clearly fallacious (no matter which modal version of the conclusion you choose). Of course, the notion of 'non-disturbance' is supposed to help patch up the argument: although the circumstances under which we can predict the value of particle 2's position are incompatible with the circumstances under which we can predict the value of particle 2's momentum, the difference between these circumstances is supposed to make no difference to particle 2.

Even with the help of some principle of non-disturbance, it is not clear, however, that EPR's argument works. Let us consider, first, a 'weak principle of non-disturbance':

> **Weak non-disturbance**: if momentum is measured on particle 1 and (therefore, by the criterion for physical reality) momentum is definite for particle 2, then: had we not measured momentum on particle 1, particle 2 would still have had a definite momentum (and likewise, substituting position for momentum).

This principle is, alas, not enough to get EPR's conclusion. They need:

> **Strong non-disturbance**: if momentum is measured on particle 1 and (therefore, by the criterion for physical reality) momentum is definite for particle 2, then: had we not measured momentum on particle 1 but instead measured its position, then particle 2 would still have had a definite momentum (and likewise, switching position and momentum).

The weak principle does not entail the strong principle because it might be impossible (without destroying essential features of the situation, in particular, our ability to infer properties of particle 2 from the results of measurements on particle 1) both to measure position on particle 1 and for momentum to be definite for particle 2. (In terms of the 'possible-worlds' semantics for counterfactuals: while the closest 'momentum is not measured'-worlds

to the ‘momentum is measured and is definite for particle 2’-worlds might all be ‘momentum is definite for particle 2’-worlds, those closest worlds may not contain any ‘position is measured’-worlds, so that the closest ‘momentum is not measured but position is’-worlds to the ‘momentum is measured and is definite for particle 2’-worlds need not be ‘momentum is definite for particle 2’-worlds. Now say that sentence three times fast.)

Bohr is sometimes understood to deny the strong principle by asserting that the act of measuring position on particle 1 ‘disturbs’ in some strange ‘semantic’ (and non-local) way the very possibility of particle 2’s having a definite momentum. Such a response is (rightly) taken to be uninteresting philosophically. In a longer account of Bohr’s reply, I would argue that while Bohr does deny the strong principle, he does so for more interesting reasons. Here, however, I shall only make a few suggestions in that direction. The next section contains several observations about EPR’s argument and Bohr’s reply. These remarks are intended to clear the air of some minor criticisms of Bohr’s reply. In the subsequent section, I shall discuss Bohr’s thought experiment and make some brief suggestions about how to understand Bohr’s reply.

2 Some Clarifications

1. *EPR speak in terms of a ‘contradiction’.* Without calling into question Fine’s (1986) logical analysis of EPR’s argument, we may note that they do speak of a ‘contradiction’ between their criterion of reality and the completeness of standard quantum theory. At the end of the first section of their paper, Einstein et al. (1935) state their conclusion thus: “We shall show, however, that this assumption [completeness], together with the criterion of reality given above, leads to a contradiction”.

As Beller and Fine (1994) argue, Bohr had no problems with EPR’s criterion for physical reality, nor with their account of completeness, together understood in a fairly conservative sense (perhaps, in modern terms, as no more than the eigenstate-eigenvalue link). Hence the idea that there might be a ‘contradiction’ between the criterion and completeness would surely have worried Bohr, and would understandably be the focus of his reply. No wonder Bohr’s rhetoric focused on ‘soundness’, ‘rationality’, ‘lack of contradiction’ and ‘consistency’ (cf. (Beller and Fine, 1994, pp. 3-4)). While we may endorse much of what Beller and Fine (1994) assert to be at the heart of Bohr’s general concerns about consistency, the simple explanation seems to be just that EPR do, at least at one point, state their conclusion in terms of a contradiction, a contradiction that was (for reasons that Beller and Fine explore) threatening to Bohr’s own position.

2. *The EPR argument focuses on the example.* I mentioned above that EPR begin their discussion in the abstract and could have finished it there, but they do not, instead resorting to the example involving position and momentum. Bohr, too, focuses on the example. Indeed, he takes the example to constitute the argument, writing that "[b]y means of an interesting example, to which we shall return below, they [EPR] next proceed to show that ... [the] formalism [of quantum mechanics] is incomplete" (Bohr, 1935, p. 696). Nobody involved in the debate seems to have thought that this focus on the example is unwarranted or misleading. The point is important for two reasons.

First, it lends greater importance to a proper understanding of Bohr's attempt to realize the example in a thought experiment. From a contemporary standpoint, one might be tempted to suppose that the real substance of the EPR argument, and of Bohr's reply, is (and was taken by them to be) in the more abstract discussions (for example, in the early part of EPR's paper and the mathematical footnote in Bohr's reply). While these more abstract discussions can provide important clues to understanding EPR and Bohr's reply, their mutual focus on the example of position and momentum suggests that we too focus on that example in order to understand what is going on.

Second, the focus on the example *is*, in the end, unwarranted and misleading. Indeed, from a contemporary standpoint, we can see that EPR chose a particularly unfortunate example to make their point. As I shall emphasize again below, the main problem is that position (unlike momentum) is not a conserved quantity, so that correlations in position will in general not be maintained under free (or for that matter, almost any other) evolution. Bohm's (1951) reworking of EPR's argument in terms of a new example (involving incompatible spin observables) fixes the problem (because spin is conserved), and it is unclear whether Bohr's reply could work in this case. (In any case, his thought experiment is mostly irrelevant to the Bohmian example.)

3. *The observables $X_1 - X_2$ and $P_1 + P_2$ can be determined simultaneously.* EPR presume that the total momentum ($P_1 + P_2$) and the distance between the particles ($X_1 - X_2$) can be known simultaneously. There is no obstacle in principle to obtaining such knowledge, since the obervables in question are compatible (mutually commuting). Indeed, the EPR state is a simultaneous eigenstate of both of these observables. (Again, we ignore the fact that plane waves and delta functions are not, strictly speaking, states, i.e., not in $L^2(R^2)$.)

But how might one actually prepare the EPR state, or more generally, how might one actually determine $X_1 - X_2$ and $P_1 + P_2$ simultaneously? That is, from a physical point of view, why do these operators commute? Note first—and this point is crucial to an understanding of Bohr's reply—that Bohr insisted that neither position nor momentum observables have

any clear physical meaning outside of the specification of some frame of reference. Bohr is acutely aware of the role that reference frames play in relativity theory, and believes that their role in the quantum theory is even more significant—well-specified frames of reference are crucial to the very meaning of 'spatial location' and 'momentum'. Bohr's view seems to have been that only prior to the discovery of the quantum theory, and specifically the 'essential exchange of momentum' involved in any interaction, could one dispense with the insistence that reference frames are essentially involved in the very notion of 'position' and 'momentum'. While a full analysis of Bohr's position on this point (and most especially of his understanding of the term 'essential exchange of momentum') is out of the question here, it is worth noting that Bohr insisted upon the necessary role that well-defined reference frames play in the very definition of the notion of position. He writes:

> Wie von EINSTEIN betont, ist es ja eine für die ganze Relativitätstheorie grundlegende Annahme, daß jede Beobachtung schließlich auf ein Zusammentreffen von Gegenstand und Meßkörper in demselben Raum-Zeitpunkt beruht und insofern von dem Bezugssystem des Beobachters unabhängig definierbar ist. Nach det Entdeckung des Wirkungsquantums wissen wir aber, daß das klassische Ideal bei der Bescreibung atomarer Vorgänge nicht erreicht werden kann. Insbesondere fürht jeder Versuch einer raum-zeitlichen Einordnung der Individuen einen Bruch der Ursachenkette mit sich, indem er mit einem nicht zu vernachlässigenden Austausch von Impuls und Energie mit den zum Vergleich benutzten Maßstäben und Uhren verbunden ist, dem keine Rechnung getragen werden kann, wenn diese Meßmittel ihren Zweck erfüllen sollen.(Bohr, 1929, p. 485)[1]

Continuing this line of thought, in his reply to EPR (1935, p. 699), Bohr writes:

> To measure the position of one of the particles can mean nothing else than to

[1] In (Bohr, 1934, pp. 97–98), the passage reads

> As Einstein has emphasized, the assumption that any observation ultimately depends upon the coincidence in space and time of the object and the means of observation and that, therefore, any observation is definable independently of the reference system of the observer is, indeed, fundamental for the whole theory of relativity. However, since the discovery of the quantum of action, we know that the classical ideal cannot be attained in the description of atomic phenomena. In particular, any attempt at an ordering in space-time leads to a break in the causal chain, since such an attempt is bound up with an essential exchange of momentum and energy between the individuals and measuring rods and clocks used for observation; and just this exchange cannot be taken into account if the measuring instruments are to fulfil their purpose.

As Michael Friedman pointed out to me, the translation does not perfectly match the original. For example, rather than "an essential exchange of momentum" one should probably say "a non-negligible [nicht zu vernachlässigenden] exchange". These subtle differences are important for a full understanding of Bohr's view and especially (perhaps) its development, but for our purposes here they are not crucial.

> establish a correlation between its behavior and some instrument rigidly fixed to the support which defines the space frame of reference.

Bohr is careful to discuss position (and momentum) in these terms, not speaking of 'the position [or momentum]' of a system, but its position relative to some other system. For example, at p. 697 of his reply he speaks not of the uncertainty of the position of a particle, but of 'the uncertainty Δq of the position of the particle relative to the diaphragm'. The fact that not only position, but also uncertainty in position, must be discussed relative to a physically defined reference frame indicates the extent to which, for Bohr, such reference frames are involved in the very meaning of 'position'.

These points are important, because failing to appreciate them fully, one can be too easily persuaded that passages such as the one above indicate Bohr's adherence to a rather strong form of operationalism. He might, in other words, be suggesting that physical properties are defined by the operations used to 'measure' them. But given the history of Bohr's insistence on the role of (physically specified) reference frames in quantum theory, we can just as well (and indeed, I would argue, more fruitfully) read the passage above and others like it as insisting that a well-defined frame of reference is crucially a part of the notion of position.

4. *The observables $X_1 - X_2$, $P_1 + P_2$, X_1, and P_1 are not mutually commuting.* It is easy to suppose that without losing our knowledge of $X_1 - X_2$ and $P_1 + P_2$, we may go on to determine either X_1 or P_1. (This mistake is all the easier if one conceives of the EPR experiment in terms of the 'standard story' that I outlined above.) The following passage, for example, seems to make this suggesstion:

> EPR consider a composite system in a state where, at least for a moment, both the relative position $X_1 - X_2$ and the total momentum $P_1 + P_2$ are co-measurable. Moreover, in EPR both of these quantities are simultaneously determinable with either the position or the momentum (not both) of particle 1. (Beller and Fine, 1994, p. 15)

However, X_1 fails to commute with $P_1 + P_2$ and P_1 fails to commute with $X_1 - X_2$. If the EPR situation allowed us to co-determine both $X_1 - X_2$ and $P_1 + P_2$ with either X_1 or P_1, then a great deal more than Bohr's reply would be in jeopardy. If we are to determine X_1, then we must give up our knowledge of $P_1 + P_2$, and if we are to determine P_1, we must give up our knowledge of $X_1 - X_2$.

As Beller (1999, ch. 6) explains, the early Bohr was very concerned to explain *why* it is not possible to observe simultaneous values for incompatible observables. I will suggest, below, that Bohr's reply to EPR continues this discussion, i.e., that he is, in part, attempting

to explain why one cannot measure $X_1 - X_2$, $P_1 + P_2$, and either of X_1 or P_1 simultaneously, within the context of EPR's example. (Here, then, is one sense in which Bohr's reply involves themes and argumentative strategies that he had already used in other cases.)

3 Bohr's Thought Experiment

We are now in a position to assess the relevance of Bohr's proposed thought experiment to EPR's argument. Bohr's discussion begins with a rehearsal of two different sorts of experiment. In the first, there is a screen with a single slit, "rigidly fixed to a support which defines the space frame of reference" (1935, p. 697), and a particle is fired at the screen. We assume that the particle's initial momentum is well-defined. Bohr asks whether, after preparing the particle in a state of well-defined position by passing it through the slit (and thereby, according to de Broglie's relation, rendering its momentum uncertain), we cannot take into account the exchange of momentum between the particle and the apparatus, thereby 'repairing' the loss of initial certainty about the momentum. His answer is 'no', because the exchange of momentum "pass[es] into this common support" which, because it *defines* the space frame of reference, *must* be taken to be at rest, and so "we have thus voluntarily [by fixing the initial screen to the support and taking that support to define the spatial reference frame] cut ourselves off from any possibility of taking these reactions separately into account" (ibid.). (Recall Bohr's claim that "just this exchange cannot be taken into account if the measuring instruments are to fulfill their purpose", quoted above.)

If, on the other hand, we allow the initial screen to move freely relative to the support, then we can indeed measure the exchange of momentum between the particle and the screen, but in so doing, we necessarily lose whatever information we might previously have had about the location of the initial screen relative to the support, and therefore passing the particle through the slit is no longer a preparation of its position relative to the support:

> In fact, even if we knew the position of the diaphragm relative to the space frame [i.e., the 'support'] before the first measurmeent of its momentum, and even though its position after the last measurement [required to determine the exchange of momentum] can be accurately fixed, we lose, on account of the uncontrollable displacement of the diaphragm during each collision process with the test bodies, the knowledge of its position when the particle passed through the slit. (1935, p. 698)

Note that two measurements of the momentum of the screen are *required* (in addition to a prior measurement of the momentum of the incident particle) in order to apply conservation

of momentum to the total system, by which we can determine the momentum of the incident particle after it has passed through the slit. Bohr claims that the second measurement of the momentum of the screen disturbs its position relative to the support in an 'uncontrollable' way, thereby preventing us from determining its position (relative to the support) at the moment that the particle passed through the slit.

My aim in making these observations is not to analyze Bohr's claims in detail. Such an analysis would include a deeper discussion of Bohr's notion of a 'reference frame', and his notion of 'uncontrollable disturbance', both of which are crucial to a complete understanding of Bohr's reply. The aim here, rather, is only to remind the reader of the broad outlines of Bohr's understanding of the uncertainty principle, and roughly how he defends that understanding by means of simple thought experiments. The main point is that Bohr believes that the 'uncontrollable exchange' of momentum and energy between a measured system and a measuring apparatus entails that those experimental situations that allow the determination of a particle's position relative to a given reference frame forbid the determination of its (simultaneous) momentum relative to that frame, and similarly, those experimental situations that allow the determination of a particle's momentum relative to a given frame—by means of an application of conservation laws—forbid the determination of its (simultaneous) position relative to that frame.

Let us turn, then, to Bohr's realization of EPR's particular case. He proposes a thought experiment to prepare the EPR state, and to perform the relevant measurements, as follows:

> The particular quantum-mechanical state of two free particles, for which they [EPR] give an explicit mathematical expression, may be reproduced, at least in principle, by a simple experimental arrangement, comprising a rigid diaphragm with two parallel slits, which are very narrow compared with their separation, and through each of which one particle with given initial momentum passes independently of the other. (Bohr, 1935, p. 699)

The arrangement as described thus far allows one to prepare the pair of particles in an eigenstate of $X_1 - X_2$, the eigenvalue being, of course, the distance between the slits (x_0 in EPR's notation). In order to determine $P_1 + P_2$, Bohr proposes the following (a continuation of the quotation above):

> If the momentum of this diaphragm is measured accurately before as well as after the passing of the particles, we shall in fact know the sum of the components perpendicular to the slits of the momenta of the two escaping particles, as well as the difference of their initial positional coordinates in the same direction. (ibid.)

Thus, at this point in the description of the thought experiment, we have determined (or prepared) the values of $X_1 - X_2$ and $P_1 + P_2$ simultaneously.

The crucial question, now, is how one may go on to measure either X_1 or P_1, in order to determine either X_2 or P_2. Concerning the measurement of X_1, Bohr begins

> [T]o measure the position of one of the particles can mean nothing else than to establish a correlation between its behavior and some instrument rigidly fixed to the support which defines the space frame of reference. Under the experimental conditions described such a measurement will therefore also provide us with the knowledge of the location, otherwise completely unknown, of the diaphragm with repect to this space frame when the particles passed through the slits. Indeed, only in this way we obtain a basis for conclusions about the initial position of the other particle relative to the rest of the apparatus. (Bohr, 1935, p. 700)

Bohr has not yet arrived at his main point, but is here pointing out that, because the initial screen must be allowed to move freely with respect to the support (so that conservation of momentum can be applied to it plus the pair of particles), we do not know where it is relative to the support until we measure the position of one of the particles (relative to the support). After such a measurement, we can learn the position of the screen, because the particles are located where the slits in the screen are located. And once we know where the screen itself is in relation to the support, we can use our knowledge of X_1 to infer the location of the other particle, as Bohr says. Note, in particular, that Bohr nowhere supposes that the measurement of the position of the particle disturbs the screen.

Bohr continues:

> By allowing an essentially uncontrollable momentum to pass from the first particle into the mentioned support, however, we have by this procedure cut ourselves off from any future possibility of applying the law of conservation of momentum to the system consisting of the diaphragm and the two particles. (ibid.)

The consequence, as Bohr notes, is that in fact we *lose* the ability to predict the momentum of the second particle, *even if* we were (counterfactually, of course) to measure the momentum of the first particle. In the terms of the first section of this essay, Bohr has rejected 'strong non-disturbance', more or less for the reason suggested there: a measurement of X_1 necessarily destroys an essential feature of the compound system prior to measurement, that feature being the truth of the conditional: if we were to measure P_1, then we could predict (with certainty) P_2. (A more complete analysis of Bohr's position would require a longer discussion of the logic of counterfactuals, which we cannot pursue here.)

From this point of view, Beller and Fine's (1994) complaints against Bohr's thought experiment are not quite right. They make two complaints. First, they are unhappy with the fact that, in Bohr's arrangement, "we have no choice but to measure X_1 at the very moment of passage of the two particles through the first diaphragm" (Beller and Fine, 1994, p. 14). As I have already pointed out, however, there is really no choice. No quantum-mechanical state can evolve into the EPR state, and the EPR state cannot be preserved by any time evolution. Hence it can be the state of a system at, and only at, the moment of preparation. We can hardly fault Bohr for this situation.

Their second complaint arises from the first. They rightly point out that Bohr does not describe in any detail how the measurement of X_1 is to occur. Indeed, straightforwrd physical consideration of the situation seems to imply that any such measurement would involve a disturbance of the diaphragm with the two slits—either indirectly (for how could one interact with the particle without 'touching' the diaphragm?) or directly, by simply fixing the diaphragm to the support. Beller and Fine appear to opt for the latter. After apparently claiming (as I noted above) that EPR's case allows for the simultaneous determination of $X_1 - X_2$, $P_1 + P_2$ *and* either X_1 or P_1, they write:

> Bohr's double slit arrangement does not satisfy this requirement. In Bohr's example only one of $X_1 - X_2$ or $P_1 + P_2$ could be co-determined together with the variable [X_1 or P_1] one chooses to measure on particle 1. Indeed, we actually have to change the set-up of the two-slit diaphragm depending on whether we intend to measure position or momentum on particle 1. In the first case the two-slit diaphragm must be immovable; in the second case it must be moveable. (1994, p. 15)

Mainly because of this situation, Beller and Fine refer to Bohr's realization of EPR's argument a "flawed assimilation of EPR to a double slit experiment" (ibid., p. 16).

I suggest an alternative account. According to this account, Bohr completely ignores the fact—even if it follows from simple physical considerations—that a measurement of X_1 implies either a disturbance of the diaphragm or that it is fixed to the support. Instead, he is concerned to point out that a measurement of X_1 involves an uncontrollable exchange of momentum between particle 1 and the support that defines the space frame of reference, in *precisely* the same way that it does in the simpler cases discussed prior to EPR. Hence the momentum of particle 1 becomes undefined, and hence the total momentum (of the pair of particles) becomes undefined. Or to put the point in more Bohrian terms: conservation of momentum cannot be applied to the compound system, and therefore $P_1 + P_2$ is undefined, because in order for it to be defined, we must be able to apply conservation of momentum to the diaphragm plus the two particles.

At the very least, this account has the merit of following quite closely Bohr's account of the disturbance. He does not say that, in the measurement of X_1, momentum is exchanged between particle 1 and the diaphragm; nor does he ever suggest, in the EPR arrangement, that the diaphragm is fixed to the support. Rather, he says that "momentum [passes] from the first particle into the mentioned support" (Bohr, 1935, p. 700).

Similarly, in his account of what goes wrong when we measure P_1, he claims that such a measurement removes the possibility of determining the location of the diaphragm relative to the support. He could have two arguments in mind. First, along lines suggested by Beller and Fine, one might argue that any measurement of P_1 must involve a disturbance of (exchange of momentum with) the diaphragm, thereby disturbing its position relative to the support, because the measurement of P_1 must occur at the moment of preparation. Second, along the lines that are suggested here, one might argue that since the arrangement *requires* the diaphragm to move freely with respect to the support (lest we be unable to determine $P_1 + P_2$), the only way to determine the location of the diaphragm relative to the support would be to measure the position of one of the particles, relative to the support. But for reasons that were discussed prior to the case of EPR, measuring P_1 'cuts one off' from the possibility of determining particle 1's (and therefore the diaphragm's) position relative to the support.

4 Bohm's version of the argument

I finish with a brief comment regarding Bohm's (1951) alternate realization of the EPR state. The main point is that Bohm's realization does not involve position and momentum, but incompatible spin observables. There are two essential differences between this case and Bohr's (and EPR's). First, spin observables, while in a sense dependent on the specification of a spatial frame of reference (because we need to know *which* direction is, for example, the 'z' direction), are not bound up as closely with the very notion of a frame of reference. In particular, the sort of exchange that must occur between particle and apparatus in a measurement of spin does not seem to involve a disturbance of the very reference frame used to define the notion of 'direction of spin'. Second, spin is a conserved quantity (unlike position), so that the measurement of spin on one particle can be made long after the preparation of the particles.

It remains to be seen whether a Bohrian response of the EPR argument can be worked out in the case of spin. My suspicion is that the Bohrian response would at the least require significant revision. As far as I am aware, Bohr never reacted, publicly or privately, to Bohm's proposed thought experiment. (And, of course, it is more or less Bohm's version that was

eventually performed.) However, the investigation of these questions must be preceded by a more complete account of Bohr's reply to EPR, to which the remarks here are at best a partial preface.

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